Comments relating to the NMFS response to Bouwes et al. 1999.

An Analysis of Differential Delayed Mortality Experienced by Stream-type Chinook Salmon of the Snake River: A response by State, Tribal, and USFWS technical staff to the 'D' analyses and discussion in the Anadromous Fish Appendix to the U.S. Army Corps of Engineers' Lower Snake River Juvenile Salmonid Migration Feasibility Study

NMFS stated in the original A-Fish Appendix that new PIT tag information indicates "recent measures" of 'D' are much higher than previously considered in the PATH analysis, and further PIT tag studies will likely resolve what is the true value of 'D'. The states, tribes and USFW responded to these claims in a paper demonstrating that 'D' is not a measurement but is instead a model estimate requiring numerous assumptions. In our paper, we explicitly describe many of these assumptions, the affect of these assumptions, and what we consider to be the most reasonable alternative for each assumption. We demonstrated that the combination of these various assumptions results in a range of possible 'D' values, depending on what you believe is the most reasonable set of alternatives. This range of 'D' values represents uncertainty in the methods used to estimate 'D' (environmental variability and measurement error were not explored in our paper).

In addition, our paper shows that the NMFS set of assumptions used to estimate 'D' resulted in a very optimistic 'D' near 0.8. NMFS implied that this value represents a measured value of 'D' under current operations. In our paper, we suggest that under a more reasonable set of assumptions, 'D' appears to be closer to 0.5. We suggest that given the range of possible assumptions and the associated variability of information used in calculating 'D' (e.g. variability around CJS survival estimates), it does not appear likely that further PIT tag information will *"resolve this question* [of what is 'D'] *in a clear and unambiguous manner"*.

The NMFS responded to the Bouwes et al. 1999 'D' paper in the revised A-Fish Appendix, Annex C and in their response to ISAB, CRITFC and IDFG comments on the first draft of the A-Fish. We regret that NMFS responded to the draft of this paper (Schaller *et al.* 1999) rather than the final version (Bouwes *et al.* 1999). The draft was sent out much earlier (August 13, 1999) to NMFS in hope to solicit comments and constructive criticisms before the final report. Because we did not receive a response, we distributed the final version of this report on October 4, 1999 (the distribution list included NMFS). Although the two drafts of the 'D' paper do differ due revisions in the first draft, the differences were small and did not change the conclusions. **This document is meant to address a few of the concerns that NMFS detailed in the recent A-Fish Appendix Annex C.**

In the Bouwes *et al.* (1999) paper we demonstrated that the high 'D' value calculated by NMFS was highly influenced by using only 2 of the upper 4 transport sites where T/C studies occurred in 1994 (LGR and LGO). The first concern NMFS describes in Annex C is the misuse of PIT tag information at the two lower projects (LMN and MCN) in describing 'D'. NMFS did not consider including the lower projects in the A-Fish Appendix for 2 reasons. The <u>first</u> reason was to be consistent with previous PATH estimates. These PATH estimates of 'D' relied on studies conducted before transportation occurred at LMN, and too few PIT-tagged fish were available in these earlier studies to evaluate transportation at MCN. PATH did not ignore transportation studies at the lower projects because they were misleading, but because the data were not available. In addition, for PATH analyses (FLUSH), T/C values for MCN were predicted separately from T/C values for Snake projects. <u>Second</u>, NMFS responded that they only used only the upper two projects because "most prospective scenarios involving transportation place

heavy emphasis on collecting and transporting fish at the upper dams." The prospective transportation scenarios that NMFS focused on in the A-Fish were A1, which includes transportation at the LGR, LGO, and LMN and A2 and A2', which includes transportation at LGR, LGO, LMN, and MCN. Although most of the transportation is meant to take place at the upper two projects, estimates of 'D' should reflect all transport projects where transportation occurs under the proposed management actions. We believe that proper evaluation of the PIT tag data will weight the fish appropriately by the proportion transported at each project.

NMFS explained in the A-FISH APPENDIX that "PIT tag results should be given substantially greater weight because the method of estimation is much improved over past methods and because they better reflect current operations." In 1994 under 'current operations', transportation occurred at LGR, LGO, LMN, and MCN , and in 1995 under 'current operations', transportation occurred at LGR, LGO, and LMN. The calculation of 'D' presented in the A-FISH APPENDIX is meant to represent the fish transported during these two years, yet the lower projects were not included in their estimate. As stated in NMFS Annex C, a greater and equal proportion of the non-tagged fish were transported at LMN and MCN (15% and 25% respectively), than at LGO (15%). Therefore, to "better reflect current operations" we do not agree that evaluating transportation at only the upper two dams best represents 'D'. Given that NMFS used a completely new approach to estimating 'D' in an attempt to capture current operations, the self imposed constraint of maintaining consistency with earlier PATH analysis by excluding information at the lower projects seems misplaced. Of course, we agree with NFMS that in order to get an average 'D' from transportation at all four projects, transport SARs must be appropriately weighted.

NMFS indicates that because PATH models are meant to represent the run-at-large it is important to prevent the possible bias of experimental (or tagged) fish misrepresenting the run-at-large (or non-tagged) fish. The argument that NMFS 'controls' do not represent the run-at-large fish in T/C experiments has been made many times in the past. And while there are still concerns over which fish represent a true in-river 'control' fish in the PIT tag studies, NMFS has made an attempt to represent the run-at-large fish with PIT tag fish by censoring fish with detection histories that do not represent the run-at-large. We agree with this approach**.** In Bouwes et al. 1999, for in-river fish, only fish with detection histories of non-detected were used in 1994 because the majority of fish that entered detection/collection facilities were transported. In 1995 and 1996, most fish entering the collection system at MCN (only) were bypassed backed to the river and thus detection histories of non-detected and detected at MCN were used to represent the non-transported run-at-large. Thus, through the proper use of detection histories, appropriate representation of the run-at-large in-river fish is possible. We note however, that in the Smith and Williams (1999) 'response to information request document' it is apparent that for fish released above LGR for 1995, NMFS also included detection histories of detected at LGR and detected at LGR and MCN without full explanation).

We are in agreement with NMFS that PIT-tagged fish transported at various projects should represent the non-PIT-tagged transported fish. Simply using all PIT-tagged fish detected at LMN and MCN to represent transported fish is misleading, because most PIT-tagged fish are returned to the river at upper projects and are not transported, unlike non-marked fish. NMFS suggests that the appropriate method for weighting fish from each collector project, for proper representation overall, is to estimate the percent of the total non-tagged fish transported at each project. It is unclear how NMFS estimated the percent of the total non-tagged fish in Annex C **(this should be clarified**), but it appears that their estimates relied on survival and FGE

estimates. We think a more straightforward approach is to use the non-detected fish, except at the site they were transported. This approach, in fact, was also described by NMFS (A-FISH APPENDIX) but only for 2 transport projects. Using fish first detected at the project where they were transported and never detected above is consistent with the approach taken for in-river fish and excludes fish returned to the river at upper transport projects. For example, in 1994 PIT-tagged fish detected only at MCN should be used to represent run-at-large fish transported at MCN, as was done in the Bouwes et al. 1999 analysis. Unfortunately, we left out some technical detail in that paper (e.g. methods described in Sanford and Smith (1999) and Smith (1999)) which lead to NMFS's misunderstanding in this area. We also note that the description in the original A-Fish does not reflect the approach used in the NMFS D-calc94-96.xls spreadsheet model. This NMFS model uses fish detected at site of transportation plus the fish detected at upstream projects and then transported at lower project.

Because we convert transported and in-river fish into LGR equivalents, as did NMFS, the number of fish transported used to determine SAR_T, is the pooled number of fish from all transport projects. Thus, the number transported at any one project relative to the pooled transported is the proportion transported at that project and is hence appropriately weighted. In 1994, the proportion of first time detects then transported (in LGR equivalents) at each transport cite is 22%, 10%, 10%, and 58% for LGR, LGS, LMN, and MCN, respectively, as reported by NMFS (see Dcalc94-96.xls), and 23%, 11%, 11%, and 54% from our data extraction. From these proportions, it becomes apparent that a significant number of smolts were transported at MCN and this should not be ignored when estimating SAR_T. These proportions are, however, quite different than the 45%, 15%, 25%, and 15% applied to the pooled SAR_T by NMFS in ANNEX C (again we note that there is no description of how NMFS estimated these per-project transport proportions in ANNEX C). Based on this comparison, either NMFS estimates of proportions are not correct, or PIT-tagged fish are more likely to end up in the collector/bypass facilities than non-tagged fish. If the latter is true, then the assumption of equal detectability of tagged and non-tagged fish of the Cormack-Jolly-Seber model is violated, indicating that PIT-tagged fish could not be used to estimate survival rates.

If NMFS suggests that the assumption of how many transport projects to include in the calculation of 'D' is resolvable by appropriately weighting the transport sites, **their A-FISH APPENDIX estimate of 1994-1995 value of 'D' should be updated to reflect this resolution.** The 1994 estimate of 'D' decrease from 1.24 to 0.82 when weighted through a procedure used by NMFS representing a 44% decrease in 'D' (ANNEX C). For ANNEX C, this decrease should have a substantial influence on their 1994-1995 estimate. However, we believe that the first-detect transport method is a more appropriate way to estimate the proportion transported at each site. In our model, this changed the 1994 NMFS 'D' estimate from 1.36 to 0.51. Using the remaining set of NMFS alternative assumptions, the pooled estimate using the 1994-1995 PIT-tag information is 0.46.

NMFS suggests that because (unweighted) SARs are 0.69%, 0.59%, 0.08%, and 0.02% at LGR, LGS, LMN, and MCN, respectively that 'D's are being driven by these SARs. Because SARs are used to calculate Ds this is true, however, this statement suggests that the low SARs at the lower projects are misleading. Lower SARs at lower projects may occur because fish that experience the stress of traveling between 3 and 4 dams before experiencing the stress of transportation may not survive as well. This also suggests that mortality due to stress in the hydrosystem may be delayed (survival in barge regardless of transport site is assumed to 0.98) and is further evidence

for hydrosystem extra mortality (see Figure 13 in Bouwes et al. 1999). **The cause of extra mortality, also a major point of our paper, was not addressed in NMFS Annex C.**

NMFS provides empirical estimates of inriver survival to BON using recent BON detectors and suggests that per-project and per-km extrapolations are similar in their accuracy. There is no description of how these survival estimates were calculated that would allow us to evaluate this claim. First, the CJS model requires another set of detections below BON to get a MCN-BON estimate. How was this done? If NMFS used trawls and terns to collect PIT tags how was the sampling random? Second, were these estimates made using Snake River origin fish? If so, how many were detected at BON, and what were the confidence intervals? Do these survival estimates represent a seasonal average based on weighted daily cohorts? In order to estimate survival to BON, something like the trawl detection system or recovery of PIT tags from tern colonies would be required to estimate lower river detections. These type of detections (trawls and terns) likely have a large number of complications associated with randomly sampling the population and/or detecting the extremely small number of Snake River fish.

If we were to evaluate the accuracy of extrapolation methods of survival estimates of spring/summer chinook to these CJS estimates, we would focus on the estimate for chinook. Chinook smolt are much smaller than steelhead smolt and may experience a higher delayed mortality from upstream experience. Migrating smolts are more subject to the higher number of predators in the lower reservoirs and projects. The problem with using a per-project expansion is that it does not allow for cumulative mortality effects, differences in reservoir length, or predator densities. The per-km expansion accounts for differences in reservoir length but does not include the other potential influences. Both expansion methods appear to over-estimate survival for chinook with the per-project much more so. This may suggest that passage models may be a more appropriate tool for extrapolation as they include differences in predator densities and composition and cumulative mortality effects. We agree that there is too little information to tell at this point, but think that being conservative is the best approach when data are limited.

NFMS suggests these uncertainties in assumptions are resolvable and ‘D’ can be measured. NMFS suggests that our analysis demonstrates that the number of transportation projects and expansion methods are the only assumptions that need to be addressed. They offer a method that should resolve the first assumption and new technology that should resolve the second assumption. While we agree with NMFS on the logic of the first assumption, we applied different methods. The new technology of BON detectors does not seem to solve the second assumption, because another reliable detection site is needed to get CJS multiple mark recapture estimates. Since NMFS (in ANNEX C) has neither presented the method for estimating survival to BON or the corresponding confidence intervals for survival, we believe their claim that this “uncertainty is solved” is still debatable. In addition, their argument on this subject, primarily based on steelhead information and on one year of chinook survival data, is equivocal. We suggest an evaluation of the assumptions associated with the Bonneville survival estimates, similar to the Bouwes et. al. 1999 ‘D’ evaluation, to determine how robust the estimates are to non-traditional detection sites and potential violations of mark recapture assumptions. Then, we could attempt to determine how likely it is that we will resolve uncertainty about ‘D’ estimates and their implementation in modeling efforts, given problems in estimating survival to BON, the numerous other assumptions, and variability in environmental conditions.

Similarly, none of there factors resolve questions about retrospective estimates of ‘D’; PATH determined this was the most sensitive component in evaluating the effectiveness of

transportation relative to drawdown. NMFS has used the 94-95 estimate of 'D' for all retrospective and prospective 'D's for some analyses and assumed 'D'=1 both retrospectively and prospectively in others. Finally, some of the assumptions in our paper were sensitive between year estimates of 'D', but cancelled out when pooled with other years. Just as it is difficult to resolve the expansion methods on 3 years of data, it is also difficult to determine which will be the most sensitive assumptions for estimates of 'D' on 3 years of data.

We may be able to come to agreement on many of these assumptions in an effort to try to come up with what we collectively believe is the best approach to measure D. And although we can not be absolutely sure whether these assumptions are correct, we will be using our best scientific judgement, which is the best we can hope for. However, we think it is misleading to suggest in the A-FISH APPENDIX that *"ongoing experiments by NMFS are likely to resolve the uncertainties regarding the differential delayed transportation mortality in 5 to 10 years"* without explicitly stating what are the assumptions in estimating 'D' and why they were chosen over other assumptions. This is the first step to resolving some of the uncertainties, but this only includes a portion of the uncertainties. Other uncertainties that have not been addressed include variability in model and measurement errors (e.g. CJS survivals) and environmental variability. These uncertainties are very large and may not be resolvable in the near term due to a variety of causes including but not restricted to insufficient sample sizes and a limited range in environmental conditions over a short time series.

Our paper stressed that two things must be true to project a moderate to high likelihood of recovery under transportation management options. For transportation options to recover listed salmon, 'D' must be high ***and*** the source of extra mortality must not be related to the hydropower system. Evidence from the recent PIT tag studies does not support either of these tenets. NMFS revised estimates of 'D', using criteria described in Annex C, would be closer to our alternative estimate of 0.48. Also, there is evidence of hydrosystem delayed mortality for in-river smolts, a point which the NMFS response did not address.

In PATH we took the approach of modeling a range of 'D' values to increase the probability of including the 'correct' D. In contrast to the comments by Karieva et al. in response to A-fish reponses, we do not think the appropriate range of 'D' values should be all-inclusive. An all inclusive range of 'D's would tell us little. As we demonstrated in our paper, while the 'D' value presented by NMFS may be possible, we think it is extremely optimistic based on other plausible assumptions. **We think that less optimistic assumptions should be given more consideration given the high risk of extinction of these stocks under current management actions.**